Comments and Suggestions for Authors

General comments:

1. Selection of Buoy Arrays:

Response: We acknowledge the importance of avoiding highly skewed buoy arrays in estimating deformation. As per your 2012 paper, we understand the advantage of using equilateral triangles to minimize errors. However, in this study, it was challenging to achieve equilateral triangle configurations due to the actual positions of the buoys. To mitigate the impact of skewness, we follow the methodology outlined in Lei et al. (2020) and Itkin et al. (2017). We believe this screening method effectively reduces the bias caused by highly skewed arrays and ensures a reliable estimation of deformation.

Reference:

Hutchings K J, Heil P, Steer A, et al. Subsynoptic scale spatial variability of sea ice deformation in the western Weddell Sea during early summer[J]. Journal of Geophysical Research: Oceans,2012.

Itkin, P., Spreen, G., Cheng, B., Doble, M., Girard-Ardhuin, F., Haapala, J., Hughes, N., Kaleschke, L., Nicolaus, M., and Wilkinson, J.: Thin ice and storms: Sea ice deformation from buoy arrays deployed during N‐ICE 2015, J. Geophys. Res. Oceans, 122, 4661-4674, https://doi.org/10.1002/2016JC012403, 2017.

Lei, R., Gui, D., Heil, P., Hutchings, J. K., and Ding, M.: Comparisons of sea ice motion and deformation, and their responses to ice conditions and cyclonic activity in the western Arctic Ocean between two summers, Cold Region Sci. Technol., 170, 102925, https://doi.org/10.1016/j.coldregions.2019.102925, 2020.

2. Calculation of Beta Values:

Response: Regarding the calculation of Beta values, we followed the method described in the referenced papers, using a least-squares fitting approach to estimate Beta from the mean deformation data. The Beta parameter was derived by fitting the relationship between the deformation rate and spatial scale, a commonly used technique in sea ice studies, such as in Stern et al. (2009). While we acknowledge your concern about the higher-than-expected Beta values, we have reviewed our results and found that these values are consistent with the methods used, and the overall trend matches other studies. Seasonal and spatial scale variations likely contribute to the differences in Beta, but the trend remains in line with previously published results.

Reference:

Lei, R., Gui, D., Heil, P., Hutchings, J. K., and Ding, M.: Comparisons of sea ice motion and deformation, and their responses to ice conditions and cyclonic activity in the western Arctic Ocean between two summers, Cold Region Sci. Technol., 170, 102925, https://doi.org/10.1016/j.coldregions.2019.102925, 2020.

3. Lagrangian Diffusion Theory:

Response: You raised a question regarding the definition and application of Lagrangian diffusion theory in our study. We clarify that our use of this theory is based on the methodology described in the references cited around line 119 of the manuscript, as well as Dawei Gui's doctoral thesis, to which we have added citations in this paper. Lagrangian diffusion theory is widely applied in atmospheric and ocean dynamics to describe flow field characteristics, and in this study, it is used to analyze sea ice motion and deformation. We will ensure that the theoretical basis and references for this method are clearly stated in the revised manuscript to avoid any ambiguity.

Reference:

Rampal, P., Bouillon, S., Olason, E., and Morlighem, M.: neXtSIM: a new Lagrangian sea ice model, Cryosphere, 10, 1055-1073, https://doi.org/10.5194/tc-10-1055-2016, 2016.

4. One-Year Data Sufficiency:

Response: We understand your concerns about the adequacy of one year of data for robust regression modelling, particularly given the inter-annual variability. The one year of data in this paper provides an initial framework, but we agree that the inclusion of more years of data will result in a more comprehensive model, particularly capturing the effects of thickness variations and climate trends. We therefore play down some of the conclusions about the universality of the findings and highlight the need for further analysis using multi-year datasets.

5. Line 25 and 29: Rampal et al. (2009) do not show that weakening of the ice, which would result in reduction of internal ice stresses, results in increased drift speeds. They simply state this as a fact which is unverified and not supported. It is a commonly held thought that because we model compressive ice strength to be a function of thickness, such that it decreases with decreasing thickness, that this will reduce internal ice stresses. This is the case in the model, but it has not been observed to my knowledge.

Response: Revised.

We have removed the attribution to Rampal et al. (2009) regarding drift speed increases, as this was not their finding.

6. line 30, page 1: The paper that should be referenced here is Marsan et al. (2004), not Stern et al. (2009).

Response: Revised.

Marsan et al. (2004) has been correctly cited instead of Stern et al. (2009).

Reference:

Marsan, D., Stern, H., Lindsay, R., and Weiss, J.: Scale dependence and localization of the deformation of Arctic sea ice. %J Physical review letters, Phys. Rev. Lett., 93, 178501, https://doi.org/10.1103/PhysRevLett.93.178501, 2004.

7. line 38-40: Hutter et al. used satellite data in their study, so it is not sensical to say their findings are consistent with satellite data as if this study was verifying the previous finding.
Response: Revised.
We have revised the statement about Hutter et al. to remove the erroneous reference to satellite data consistency.

8. line 41-42: You reference a paper for no apparent reason. While Hibler et al. 2006 is interesting (I might be biased, I was the second author and wrote the text of this paper), multi-equilibrium flow states has nothing to do with the narrative of your paper.
Response: Revised.
We agree that Hibler et al. (2006) does not fit within the context of our discussion and have removed this citation.

Specific comments:
1. Abstract line 9: Word choice: Sea ice does not govern climate. Perhaps a better word here is "regulates", but you might want to think through the role sea ice has in the climate system in choosing an appropriate word here.
Response: Revised.

2. line 29: positively is an ambiguous word choice. How is this a positive relationship?
Response: Revised.
We revised the ambiguous word "positively" to make the relationship clearer.

3. line 59: An example of overly general language that conveys little meaning as to what you did. There are several places this is an issue in the paper. "stdies on the factors affecting sea ice deformation characteristics at large ranges of spatial and temporal scales are limited". Which factors? which characteristics? You do not introduce even what limited work has been done and what factors need to be considered.

Response: Revised.
We have deleted this sentence.

4. section 2.2, first paragraph: There is another method that has been recently developed.
Response: Revised.
We have added the methodology of this literature to the paper.

Reference:
Aksamit, N. O., Scharien, R. K., Hutchings, J. K., & Lukovich, J. V. (2023). A quasi-objective single-buoy approach for understanding Lagrangian coherent structures and sea ice dynamics. The Cryosphere, 17(4), 1545-1566.

5. You are calling the maximum shear simply as shear. It would be more correct to refer to the value calculated in equation 7 as the maximum shear.

6. Figure 4: You could include a line for your definition of ice free.
Response: Revised.
We have added a definition of ‘ice free’.

7. section 3.3. introductory paragraph. This is out of place and should be at the front of the paper in the introduction.
Response: Revised.
We have placed the introductory paragraph in the introduction of the paper.

8. lines 265-270: Contradictory sentences and meaning mangled.
Response: Revised.

9. line 300: Very confusing sentence.
Response: Revised.
We've changed the expression.

10. line 319: What is the "critical value", as in how do you define this?
Response: The critical value of 1.35 m was chosen as the median sea ice thickness.